SCIENCE

NEW SERIES
VOL. LII, No. 1342

FRIDAY, SEPTEMBER 17, 1920

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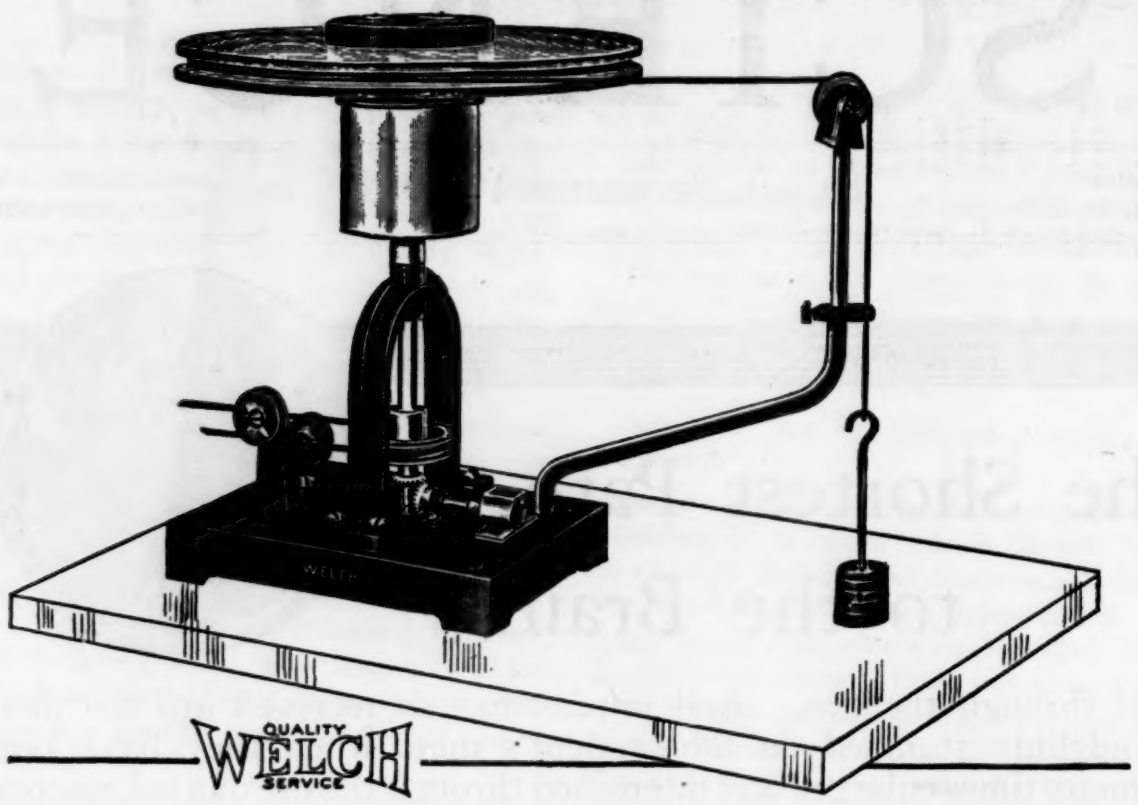

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FOSSILS AND LIFE[1]

THE DIFFERENTIA OF PALEONTOLOGY

LIKE botany and zoology, paleontology describes the external and internal form and structure of animals and plants; and on this description it bases, first, a systematic classification of its material; secondly, those broader inductions of comparative anatomy which constitute morphology, or the science of form. Arising out of these studies are the questions of relation—real or apparent kinship, lines of descent, the how and the why of evolution—the answers to which reflect their light back on our morphological and classificatory systems. By a different approach we map the geological distribution of genera and species, thus helping to elucidate changes of land and sea, and so barring out one hypothesis of racial descent or unlocking the door to another. Again, we study collective faunas and floras, unravelling the interplay of their component animals and plants, or inferring from each assemblage the climatic and other physical agents that favored, selected, and delimited it.

All this, it may be said, is nothing more than the botany and zoology of the past. True, the general absence of any soft tissues, and the obscured or fragmentary condition of those harder parts which alone are preserved, make the studies of the paleontologist more difficult, and drive him to special methods. But the result is less complete: in short, an inferior and unattractive branch of biology. Let us relegate it to Section C!

Certainly the relation of paleontology to geology is obvious. It is a part of that general history of the earth which is geology. And it is an essential part even of physical geology, for without life not merely would our series of strata have lacked the coal measures, the

[1] From the address of the president of the Geological Section of the British Association for the Advancement of Science, Cardiff, 1920.

mountain limestones, the chalks, and the siliceous earths, but the changes of land and sea would have been far other. To the scientific interpreter of earth-history, the importance of fossils lies first in their value as date-markers; secondly, in the light which they cast on barriers and currents, on seasonal and climatic variation. Conversely, the history of life has itself been influenced by geologic change. But all this is just as true of the present inhabitants of the globe as it is of their predecessors. It does not give the *differentia* of paleontology.

That which above all distinguishes paleontology—the study of ancient creatures, from neontology—the study of creatures now living, that which raises it above the mere description of extinct assemblages of life-forms, is the concept of time. Not the quasi-absolute time of the clock, or rather of the sun; not various unrelated durations; but an orderly and related succession, coextensive, in theory at least, with the whole history of life on this planet. The bearing of this obvious statement will appear from one or two simple illustrations.

EFFECT OF THE TIME-CONCEPT ON PRINCIPLES OF CLASSIFICATION

Adopting the well-tried metaphor, let us imagine the tree of life buried, except for its topmost twigs, beneath a sand-dune. The neontologist sees only the unburied twigs. He recognizes certain rough groupings, and constructs a classification accordingly. From various hints he may shrewdly infer that some twigs come from one branch, some from another; but the relations of the branches to the main stem are matters of speculation, and when branches have become so interlaced that their twigs have long been subjected to the same external influences, he will probably be led to incorrect conclusions. The paleontologist then comes, shovels away the sand, and by degrees exposes the true relations of branches and twigs. His work is not yet accomplished, and probably he never will reveal the root and lower part of the tree; but already he has corrected many natural, if not inevitable, errors of the neontologist.

I could easily occupy the rest of this hour by discussing the profound changes wrought by this conception on our classification. It is not that orders and classes hitherto unknown have been discovered, not that some erroneous allocations have been corrected, but the whole basis of our system is being shifted. So long as we were dealing with a horizontal section across the tree of life—that is to say, with an assemblage of approximately contemporaneous forms—or even with a number of such horizontal sections, so long were we confined to simple description. Any attempt to frame a causal connection was bound to be speculative. Certain relations of structure, as of cloven hoofs with horns and with a ruminant stomach, were observed, but, as Cuvier himself insisted, the laws based on such facts were purely empirical. Huxley, then, was justified in maintaining, as he did in 1863 and for long after, that a zoological classification could be based with profit on "purely structural considerations" alone. "Every group in that [kind of] classification is such in virtue of certain structural characters, which are not only common to the members of that group, but distinguish it from all others; and the statement of these constitutes the definition of the group." In such a classification the groups or categories—from species and genera up to phyla—are the expressions of an arbitrary intellectual decision. From Linnæus downwards botanists and zoologists have sought for a classification that should be not arbitrary but natural, though what they meant by "natural" neither Linnæus nor his successors either could or would say. Not, that is, until the doctrine of descent was firmly established, and even now its application remains impracticable, except in those cases where sufficient proof of genetic connection has been furnished —as it has been mainly by paleontology. In many cases we now perceive the causal connection; and we recognize that our groupings, so far as they follow the blood-red clue, are not arbitrary but tables of natural affinity.

Fresh difficulties, however, arise. Consider

the branching of a tree. It is easy to distinguish the twigs and the branches each from each, but where are we to draw the line along each ascending stem? To convey the new conception of change in time we must introduce a new set of systematic categories, called grades or series, keeping our old categories of families, orders, and the like for the vertical divisions between the branches. Thus, many crinoids with pinnulate arms arose from others in which the arms were non-pinnulate. We can not place them in an order by themselves, because the ancestors belonged to two or three orders. We must keep them in the same orders as their respective ancestors, but distinguish a grade Pinnata from a grade Impinnata.

This sounds fairly simple, and for the larger groups so it is. But when we consider the genus, we are met with the difficulty that many of our existing genera represent grades of structure affecting a number of species, and several of those species can be traced back through previous grades. This has long been recognized, but I take a modern instance from H. F. Osborn's "Equidæ":[2]

> The line between such species as *Miohippus* (*Mesohippus*) *meteulophus* and *M. brachystylus* of the Leptauchenia zone and *M.* (*Mesohippus*) *intermedius* of the Protoceras zone is purely arbitrary. It is obvious that members of more than one phylum [*i. e.*, lineage] are passing from one genus into the next, and *Mesohippus meteulophus* and *M. brachystylus* may with equal consistency be referred to *Miohippus*.

The problem is reduced to its simplest elements in the following scheme:

a	*b*	*c*	*d*	*e*	*f*	Italics.
a	b	c	d	e	f	Lower-case Romans.
A	B	C	D	E	F	Capital Romans.
α	β	γ	δ	ϵ	ϕ	Greek.

Our genera are equivalent to the forms of letters: Italics, Roman, Greek, and so forth. The successive species are the letters themselves. Are we to make each species a genus? Or would it not be better to confess that here, as in the case of many larger groups, our basis of classification is wrong? For the paleontologist, at any rate, the lineage *a*, A, a, *a*, is the all-important concept. Between these forms he finds every gradation; but between *a* and *b* he perceives no connection.

In the old classification the vertical divisions either were arbitrary, or were gaps due to ignorance. We are gradually substituting a classification in which the vertical divisions are based on knowledge, and the horizontal divisions, though in some degree arbitrary, often coincide with relatively sudden or physiologically important changes of form.

This brings us to the last point of contrast. Our definitions can no longer have the rigid character emphasized by Huxley. They are no longer purely descriptive. When it devolved on me to draw up a definition of the great Echinoderma, a definition that should include all the fossils, I found that scarcely a character given in the text-books could certainly be predicated of every member of the group. The answer to the question, "What is an Echinoderm?" (and you may substitute Mollusc, or Vertebrate, or what name you please) has to be of this nature: An Echinoderm is an animal descended from an ancestor possessed of such-and-such characters differentiating it from other animal forms, and it still retains the imprint of that ancestor, though modified and obscured in various ways according to the class, order, family, and genus to which it belongs. The definitions given by Professor Charles Schuchert in his classification of the Brachiopoda[3] represent an interesting attempt to put these principles into practise. The Family Porambonitidæ, for instance, is thus defined:

> Derived (out of Syntrophiidæ), progressive, semirostrate Pentamerids, with the deltidia and chilidia vanishing more and more in time. Spondylia and cruralia present, but the former tends to thicken and unite with the ventral valve.

The old form of diagnosis was *per genus et differentiam*. The new form is *per proavum et modificationem*.

Even the conception of our fundamental

[2] 1918, *Mem. Amer. Mus. N. H.*, N. S., II., 51.

[3] 1913, Eastman's "Zittel."

unit, the species, is insecure owing to the discovery of gradual changes. But this is a difficulty which the paleontologist shares with the neontologist.

Let us consider another way in which the time-concept has affected biology.

EFFECT OF THE TIME-CONCEPT ON IDEAS OF RELATIONSHIP

Etienne Geoffroy-Saint Hilaire was the first to compare the embryonic stages of certain animals with the adult stages of animals considered inferior. Through the more precise observations of Von Baer, Louis Agassiz, and others, the idea grew until it was crystallized by the poetic imagination of Haeckel in his fundamental law of the reproduction of life—namely, that every creature tends in the course of its individual development to pass through stages similar to those passed through in the history of its race. This principle is of value if applied with the necessary safeguards. If it was ever brought into disrepute, it was owing to the reckless enthusiasm of some embryologists, who unwarrantably extended the statement to all shapes and structures observed in the developing animal, such as those evoked by special conditions of larval existence, sometimes forgetting that every conceivable ancestor must at least have been capable of earning its own livelihood. Or, again, they compared the early stages of an individual with the adult structure of its contemporaries instead of with that of its predecessors in time. Often, too, the searcher into the embryology of creatures now living was forced to study some form that really was highly specialized, such as the unstalked Crinoid *Antedon,* and he made matters worse by comparing its larvæ with forms far too remote in time. Allman, for instance, thought he saw in the developing *Antedon* a Cystid stage, and so the Cystids were regarded as the ancestors of the Crinoids; but we now find that stage more closely paralleled in some Crinoids of Carboniferous and Permian age, and we realize that the Cystid structure is quite different.

Such errors were due to the ignoring of time relations or to lack of acquaintance with extinct forms, and were beautifully illustrated in those phylogenetic trees which, in the 'eighties, every dissector of a new or striking animal thought it his duty to plant at the end of his paper. The trees have withered, because they were not rooted in the past.

A similar mistake was made by the paleontologist who, happening on a new fossil, blazoned it forth as a link between groups previously unconnected—and in too many cases unconnected still. This action, natural and even justifiable under the old purely descriptive system, became fallacious when descent was taken as the basis. In those days one heard much of generalized types, especially among the older fossils; animals were supposed to combine the features of two or three classes. This mode of thought is not quite extinct, for in the last American edition of Zittel's "Paleontology" *Stephanocrinus* is still spoken of as a Crinoid related to the Blastoids, if not also to the Cystids. Let it be clear that these so-called "generalized" or "annectant" types are not regarded by their expositors as ancestral. Of course, a genus existing at a certain period may give rise to two different genera of a succeeding period, as possibly the Devonian *Cœlocrinus* evolved into *Agaricocrinus,* with concave base, and into *Dorycrinus,* with convex base, both Carboniferous genera. But, to exemplify the kind of statement here criticized, perhaps I may quote from another distinguished writer of the present century:

> The new genus is a truly annectant form uniting the Melocrinidæ and the Platycrinidæ.

Now the genus in question appeared, so far as we know, rather late in the Lower Carboniferous, whereas both Platycrinidæ and Melocrinidæ were already established in Middle Silurian time. How is it possible that the far later form should unite these two ancient families? Even a *mésalliance* is inconceivable. In a word, to describe any such forms as "annectant" is not merely to misinterpret structure but to ignore time.

As bold suggestions calling for subsequent proof these speculations had their value, and they may be forgiven in the neontologist, if

not in the paleontologist, if we regard them as erratic pioneer tracks blazed through a tangled forest. As our acquaintance with fossils enlarged, the general direction became clearer, and certain paths were seen to be impossible. In 1881, addressing this association at York, Huxley could say:

> Fifty years hence, whoever undertakes to record the progress of paleontology will note the present time as the epoch in which the law of succession of the forms of the higher animals was determined by the observation of paleontological facts. He will point out that, just as Steno and as Cuvier were enabled from their knowledge of the empirical laws of coexistence of the parts of animals to conclude from a part to a whole, so the knowledge of the law of succession of forms empowered their successors to conclude, from one or two terms of such a succession, to the whole series, and thus to divine the existence of forms of life, of which, perhaps, no trace remains, at epochs of inconceivable remoteness in the past.

DESCENT NOT A COROLLARY OF SUCCESSION

Note that Huxley spoke of succession, not of descent. Succession undoubtedly was recognized, but the relation between the terms of the succession was little understood, and there was no proof of descent. Let us suppose all written records to be swept away, and an attempt made to reconstruct English history from coins. We could set out our monarchs in true order, and we might suspect that the throne was hereditary; but if on that assumption we were to make James I. the son of Elizabeth—well, but that's just what paleontologists are constantly doing. The famous diagram of the Evolution of the Horse which Huxley used in his American lectures has had to be corrected in the light of the fuller evidence recently tabulated in a handsome volume by Professor H. F. Osborn and his coadjutors. *Palæotherium,* which Huxley regarded as a direct ancestor of the horse, is now held to be only a collateral, as the last of the Tudors were collateral ancestors of the Stuarts. The later *Anchitherium* must be eliminated from the true line as a side-branch—a Young Pretender. Sometimes an apparent succession is due to immigration of a distant relative from some other region—"The glorious House of Hanover and Protestant Succession." It was, you will remember, by such migrations that Cuvier explained the renewal of life when a previous fauna had become extinct. He admitted succession but not descent. If he rejected special creation, he did not accept evolution.

Descent, then, is not a corollary of succession. Or, to broaden the statement, history is not the same as evolution. History is a succession of events. Evolution means that each event has sprung from the preceding one. Not that the preceding event was the active cause of its successor, but that it was a necessary condition of it. For the evolutionary biologist, a species contains in itself and its environment the possibility of producing its successor. The words "its environment" are necessary, because a living organism can not be conceived apart from its environment. They are important, because they exclude from the idea of organic evolution the hypothesis that all subsequent forms were implicit in the primordial protoplast alone, and were manifested either through a series of degradations, as when thorium by successive disintegrations transmutes itself to lead, or through fresh developments due to the successive loss of inhibiting factors. I say "a species contains the possibility" rather than "the potentiality," because we can not start by assuming any kind of innate power.

Huxley, then, forty years ago, claimed that paleontologists had proved an orderly succession. To-day we claim to have proved evolution by descent. But how do we prove it? The neontologist, for all his experimental breeding, has scarcely demonstrated the transmutation of a species. The paleontologist can not assist at even a single birth. The evidence remains circumstantial.

RECAPITULATION AS PROOF OF DESCENT

Circumstantial evidence is convincing only if inexplicable on any other admissible theory. Such evidence is, I believe, afforded by paleontological instances of Haeckel's law—*i. e.,* the recapitulation by an individual during its growth of stages attained by adults in the

previous history of the race. You all know how this has been applied to the ammonites; but any creatures with a shell or skeleton that grows by successive additions and retains the earlier stages unaltered can be studied by this method. If we take a chronological series of apparently related species or mutations, a^1, a^2, a^3, a^4, and if in a^4 we find that the growth stage immediately preceding the adult resembles the adult a^3, and that the next preceding stage resembles a^2, and so on; if this applies *mutatis mutandis* to the other species of the series; and if, further, the old age of each species foreshadows the adult character of its successor; then we are entitled to infer that the relation between the species is one of descent. Mistakes are liable to occur for various reasons, which we are learning to guard against. For example, the perennial desire of youth to attain a semblance of maturity leads often to the omission of some steps in the orderly process. But this and other eccentricities affect the earlier rather than the later stages, so that it is always possible to identify the immediate ancestor, if it can be found. Here we have to remember that the ancestor may not have lived in the same locality, and that therefore a single cliff-section does not always provide a complete or simple series. An admirable example of the successful search for a father is provided by R. G. Carruthers in his paper on the evolution of *Zaphrentis delanouei*.[4] Surely when we get a clear case of this kind we are entitled to use the word "proof," and to say that we have not merely observed the succession, but have proved the filiation.

It has, indeed, been objected to the theory of recapitulation that the stages of individual growth are an inevitable consequence of an animal's gradual development from the embryo to the adult, and therefore prove nothing. Even now there are those who maintain that the continuity of the germ-plasm is inconsistent with any true recapitulation. Let us try to see what this means. Take any evolutionary series, and consider the germ-plasm at any early stage in it. The germ, it is claimed, contains the factors which produce the adult characters of that stage. Now proceed to the next stage of evolution. The germ has either altered or it has not. If it has not altered, the new adult characters are due to something outside the germ, to factors which may be in the environment but are not in the germ. In this case the animal must be driven by the inherited factors to reproduce the ancestral form; the modifications due to other factors will come in on the top of this, and if they come in gradually and in the later stages of growth, then there will be recapitulation. There does not seem to be any difficulty here. You may deny the term "character" to these modifications, and you may say that they are not really inherited, that they will disappear entirely if the environment reverts to its original condition. Such language, however, does not alter the fact, and when we pass to subsequent stages of evolution and find the process repeated, and the recapitulation becoming longer, then you will be hard put to it to imagine that the new environment produces first the effects of the old and then its own particular effect.

Even if we do suppose that the successive changes in, say, an ammonite as it passes from youth to age are adaptations to successive environments, this must mean that there is a recapitulation of environment. It is an explanation of structural recapitulation, but the fact remains. There is no difficulty in supposing an individual to pass through the same succession of environments as were encountered in the past history of its race. Every common frog is an instance. The phenomenon is of the same nature as the devious route followed in their migrations by certain birds, a route only to be explained as the repetition of past history. There are, however, many cases, especially among sedentary organisms, which can not readily be explained in this way.

Let us then examine the other alternative and suppose that every evolutionary change is due to a change in the germ—how produced we need not now inquire. Then, presumably, it is claimed that at each stage of evolution the animal will grow from the egg to the adult

[4] 1910, *Quart. Jour. Geol. Soc.*, LXVI., 523.

along a direct path. For present purposes we ignore purely larval modifications, and admit that the claim appears reasonable. The trouble is that it does not harmonize with facts. The progress from youth to age is not always a simple advance. The creature seems to go out of its way to drag in a growth-stage that is out of the straight road, and can be explained only by the fact that it is inherited from an ancestor. Thus, large ammonites of the *Xipheroceras planicosta* group, beginning smooth, pass through a ribbed stage, which may be omitted, through unituberculate and bituberculate stages, back to ribbed and smooth again. The anal plate of the larval *Antedon,* which ends its course and finally disappears above the limits of the cup, begins life in that lower position which the similar plate occupied in most of the older crinoids.

Here, then, is a difficulty. It can be overcome in two ways. A view held by many is that there are two kinds of characters: first, those that arise from changes in the germ, and appear as sudden or discontinuous variations; second, those that are due to external (*i. e.,* non-germinal) factors. It seems a corollary of this view that the external characters should so affect the germ-plasm as ultimately to produce in it the appropriate factors. This is inheritance of acquired characters. The other way out of the difficulty is to suppose that all characters other than fluctuations or temporary modifications are germinal; that changes are due solely to changes in the constitution of the germ; and that, although a new character may not manifest itself till the creature has reached old age, nevertheless it was inherent in the germ and latent through the earlier growth-stages. This second hypothesis involves two further difficulties. It is not easy to formulate a mechanism by which a change in the constitution of the germ shall produce a character of which no trace can be detected until old age sets in; such a character, for instance, as the tuberculation of the last-formed portion of an ammonite shell. Again, it is generally maintained that characters due to this change of germinal factors, however minute they may be, make a sudden appearance. They are said to be discontinuous. They act as integral units. Now the characters we are trying to explain seem to us paleontologists to appear very gradually, both in the individual and in the race. Their beginnings are small, scarcely perceptible; they increase gradually in size or strength; and gradually they appear at earlier and earlier stages in the life-cycle. It appears least difficult to suppose that characters of this kind are not initiated in the germ, and that they, if no others, may be subject to recapitulation. It may not yet be possible to visualize the whole process by which such characters are gradually established, or to refer the phenomena of recapitulation back to more fundamental principles. But the phenomena are there, and if any hypothesis is opposed to them so much the worse for the hypothesis. However they be explained, the instances of recapitulation afford convincing proof of descent, and so of genetic evolution.

THE "LINE UPON LINE" METHOD OF PALEONTOLOGY

You will have observed that the precise methods of the modern paleontologist, on which this proof is based, are very different from the slap-dash conclusions of forty years ago. The discovery of *Archæopteryx,* for instance, was thought to prove the evolution of birds from reptiles. No doubt it rendered that conclusion extremely probable, especially if the major premiss—that evolution *was* the method of nature—were assumed. But the fact of evolution is precisely what men were then trying to prove. These jumpings from class to class or from era to era, by aid of a few isolated stepping-stones, were what Bacon calls anticipations, "hasty and premature" but "very effective, because as they are collected from a few instances, and mostly from those which are of familiar occurrence, they immediately dazzle the intellect and fill the imagination."[5] No secure step was taken until the modern paleontologists began to affiliate mutation with mutation and species with species, working his

[5] *Nov. Org.,* I., 28.

way back, literally inch by inch, through a single small group of strata. Only thus could he base on the laboriously collected facts a single true interpretation; and to those who preferred the broad path of generality his interpretations seemed, as Bacon says they always "must seem, harsh and discordant—almost like mysteries of faith."

It is impossible to read these words without thinking of one "naturæ minister et interpres," whose genius was the first in this country to appreciate and apply to paleontology the Novum Organon. Devoting his whole life to abstruse research, he has persevered with this method in the face of distrust and has produced a series of brilliant studies which, whatever their defects, have illuminated the problems of stratigraphy and gone far to revolutionize systematic paleontology. Many are the workers of to-day who acknowledge a master in Sydney Savory Buckman.

I have long believed that the only safe mode of advance in paleontology is that which Bacon counselled and Buckman has practised, namely, "uniformly and step by step." Was this not indeed the principle that guided Linnæus himself? Not till we have linked species into lineages, can we group them into genera; not till we have unravelled the strands by which genus is connected with genus can we draw the limits of families. Not till that has been accomplished can we see how the lines of descent diverge or converge, so as to warrant the establishment of orders. Thus by degrees we reject the old slippery stepping-stones that so often toppled us into the stream, and foot by foot, we build a secure bridge over the waters of ignorance.

Francis Arthur Bather

ATLANTIC AND PACIFIC SALMON

History repeats itself with monotonous regularity and the most patent facts of scientific knowledge apparently make no impression on the people at large even where their own interests are vitally concerned. They try over and over the same experiment and after the clearly foretold results have been secured they lament the unfortunate consequences. Not only that but an expenditure of money to improve the situation is often rendered useless by action which passes without adequate protest from those most immediately interested.

In former centuries the Atlantic salmon ran yearly in the rivers of the New England coast in such numbers as to excite the amazement of our forefathers. They thought the supply inexhaustible but in 1798 a dam was erected on the Connecticut River and the results are thus described by Jordan and Evermann.

> The salmon was at one time very abundant in the Connecticut, and it probably occurred in the Housatonic and Hudson. . . . The circumstances of their extermination in the Connecticut are well known, and the same story, with names and dates changed, serves equally well for other rivers.
>
> In 1798 a corporation known as the Upper Locks and Canal Company built a dam 16 feet high at Millers River, 100 miles from the mouth of the Connecticut. For two or three years fish were seen in great abundance below the dam, and for perhaps ten years they continued to appear, vainly striving to reach their spawning grounds; but soon the work of extermination was complete. When, in 1872, a solitary salmon made its appearance, the Saybrook fishermen did not know what it was.

The experiment has been tried in many other places and each time the result has been the same. We have heard much in recent years about the dangers confronting the Pacific salmon which furnishes so important a part of the food supply of this country and of other parts of the world. Scientific men have called attention to the serious dangers which ill-considered promotion and careless destruction of spawning grounds have brought to bear on the supply of this splendid fish.

In response to these warnings President Roosevelt appointed a commission for the investigation of problems connected with the Pacific salmon and its fisheries and Congress has continued the work of studying the situation and of aiding the fish to maintain its position by the establishment and development of hatcheries. One of the oldest and most prominent is at Baird, California. It is accordingly

with grave apprehension that I have read the following paragraph in a recent publication.

> Only a few spring-run fish have been in McCloud River at Baird, California, and the dam without a fishway in the Sacramento River is to a considerable extent responsible for the condition which threatens to render the Baird hatchery useless.

In California certain state officials have suggested that since the dam was constructed without a permit from the War Department, action to correct the evil should be taken by the United States authorities. But since the Sacramento River at the point in question has not been adjudged a navigable stream, no permit was required and the matter falls legally wholly under the control of the state of California. It is pertinent to ask whether that state is so lacking in foresight and its officers so devoid of responsibility for public interests that they will continue to permit conditions that menace thus directly the public welfare.

But the question has an even broader aspect. These fish are a national asset. They are born in the waters of an individual state but they soon pass into the ocean, glean from it without expense to any state or nation the supply of energy that brings them back at stated periods to contribute to individual enterprise and to national food supply a harvest that is of all which man gathers the most profitable because it demands least care and utilizes for its production otherwise unused sources of energy.

The nation is vitally concerned with impending danger. It has contributed the means by which the hatchery is maintained and it has a moral if not a full legal right to see that no private agencies thus in irresponsible manner destroy the results of its efforts. Some way should be found and some agency invigorated to the point where it will insist upon the maintenance of proper fishways even though this involve expense upon the interests involved.

This is, however, only one phase of a question which has many aspects. The run of Pacific salmon has entirely disappeared in some streams. In others it has been tremendously impaired. In districts like Puget Sound it has sunk to a fraction of its former size and during 1919 only one district in Alaska reported a catch that equalled 100 per cent. of the number for the preceding ten years. Furthermore these results were obtained by the use of more boats, more men, more gear and other destructive appliances than had ever been in service before.

In his latest report the United States Commissioner of Fisheries calls attention to the situation in so far as it concerns Alaska waters and the salmon therein, in the following terms:

> For about eight years legislation affecting the fisheries of Alaska has been pending in Congress. Protracted hearings have been presented to the appropriate committees of the two houses. The necessity for a radical revision of the existing salmon law has been especially pointed out by various agencies and persons interested in the welfare of the fisheries of Alaska, and congressional committees have made favorable reports on bills embodying new legislation.
>
> No new fishery laws have, however, been enacted; and the fisheries of Alaska, at the most critical period of their history, remain subject to laws which have been shown to be obsolete and inadequate. The Bureau of Fisheries is thus placed at a great disadvantage in administering the salmon fisheries of Alaska and can not justly be held accountable for conditions, practises and developments which, while having the full sanction of law, are not necessarily compatible with the perpetuation of the supply and in some respects are directly opposed thereto.

Concerning the magnitude of the problem the same report speaks in another place thus:

> It is the salmon industry which gives to the fisheries of Alaska their great importance, and it was the salmon industry that contributed most notably to the increases that occurred in 1918. The value of all salmon products was $53,514,812, of which $51,041,949, represented canned fish to the number of 6,605,835 cases. Thus, 50 years after Alaska became a part of our national domain, the salmon resources alone yielded a product valued at over 7½ times the purchase price of the territory.

The public interest thus put in jeopardy is of the first magnitude and the danger both

real and immediate. Biologists know how rapidly the progress of destruction proceeds and how soon the end comes when the diminution in numbers of any species has once become conspicuous. Increasing values always lead to redoubled efforts and multiplied appliances for securing a catch and the vicious cycle gains in velocity as it decreases in diameter.

The commercial interests are strangling the goose that has laid for them so many golden eggs and some are beginning to be apprehensive for the future. Unless public sentiment can be developed, unless the efforts of the Bureau of Fisheries can be supported by adequate appropriations, and unless the taking of salmon can be subjected to reasonable restrictions that splendid fish will in a short time be as much of a luxury on the Pacific coast as its congenér is to-day on the Atlantic.

Henry B. Ward

JOSEPH PANTEL (1853–1920), A JESUIT AND A SCIENTIST

The first days of last February saw the closing of a remarkable scientific career, when the death of Fr. Pantel, S.J., occurred in Toulouse, France. By dint of tireless work and scientific investigations, he had gained for himself an international reputation. He had developed a keen power of observation and was considered by all the men of science who knew him as a first class biologist.

Twice the Academy of Sciences of Paris recognized the merits of his works. In 1898, it conferred on him the "Thore Prize" for his masterly monograph on the "*Thrixion halidayanum*"; in 1906, the "Gama Machado Prize" for his studies on the cells of the masculine type of the "*Notonecta glauca.*" This second time the work had been done in collaboration with his disciple, Rev. Robert de Sinéty, S.J.

In 1891–92 he studied at Louvain University in the Carnoy Laboratory. In later years the systematic study of the Coleoptera and Orthoptera, their anatomy and biology, constituted the main guiding lines of his research work. The observations and discoveries he made were published by several scientific magazines, *La Cellule* and *Le Neuraxe,* of Louvain, and by many scientific societies, The Royal Spanish Society of Natural History, The Royal Academy of Arts and Sciences of Barcelona, the Entomological Societies of France, Holland and others.

The most famous entomologists of Europe revered his learning and admired his modesty. In his honor they designated several new species with the name of "*Panteli*" and several genera—a genus of Orthoptera "*Pantelia*" (Bolivar) and a genus of Diptera "*Panteliola*" (Rev. Kieffer). Dr. P. Halbfass, teaching in the University of Munster in 1903, spoke of Fr. Pantel in glowing terms of admiration. Being a Lutheran, he did not mention to the class that Fr. Pantel was a Jesuit but he confessed that he had been sent to study under his direction by his well-known professor, Dr. O. Hertwig, who saw in Fr. Pantel an eminent scientist.

Parasitism among insects, customs and social habits of the pirates and robbers among the winged tribe, the shameless impudence of beggarly insects which cause the death of others in order that they may live—such were the topics of interest in the studies of Fr. Pantel.

After the war the services of Fr. Pantel as professor were enlisted in the Catholic Scientific Institute of Toulouse. The writer can testify that, in the work of the class-room, Fr. Pantel was equally eminent. His lectures were always very carefully prepared and delivered, the right word always in the right place. No unnecessary repetitions that tire the mind or distract the attention. Fr. Pantel gave his lectures, guided by psychological principles and never did spare labor to obtain the best results in the shortest time. As an instance of the last assertion the writer may adduce the fact of Fr. Pantel's spending in his early career 17 hours work for the preparation of a class-room lecture lasting 55 minutes.

The number of students who came to attend his lectures was a silent testimony of the value of his teaching. They came from Spain, Por-

tugal, Italy, Germany, England, Ireland, Belgium, South America and even remote Australia.

J. H. FOULQUIER, S.J.
SACRED HEART COLLEGE,
DENVER, COLO.

SCIENTIFIC EVENTS

TRIBUTE TO THE MEMORY OF JAMES WILSON

SYMPATHY at the death of former Secretary of Agriculture James Wilson was sent to his family in the form of a resolution adopted at a meeting of the chiefs of the various bureaus of the United States Department of Agriculture. Tribute was paid to the former head of the department for "his patriotic devotion to the interests of all the people, his broad vision, and his practical wisdom." As a token of respect the flags on all department buildings were placed at halfstaff, and remained so until after the funeral, which took place at Traer, Iowa.

Because of the time of the funeral, the department was unable to send representatives from Washington. The department, however, designated Dr. Henry C. Taylor, Chief of the Office of Farm Management, who was in the Middle West; Frank S. Pinney, Federal agricultural statistician at Des Moines; and R. E. Doolittle, Chief of the Central Food and Drug Inspection District at Chicago, to represent it at the funeral.

A floral tribute was sent by officials and employees of the department as a token of esteem for their former chief. The message of sympathy sent the family of Mr. Wilson followed a similar personal message sent by Secretary of Agriculture Meredith. The resolution of the bureau chiefs, forwarded by Assistant Secretary of Agriculture Ball, read:

The members of the Department of Agriculture, feeling deeply the loss of their former secretary, James Wilson, of Iowa, desire to express their sympathy with his family and their appreciation of his great services to the United States as Dean of Agriculture, member of Congress, and Secretary of Agriculture. His patriotic devotion to the interests of all the people, his broad vision, and his practical wisdom place him high among those who have deserved well of their country. Beloved as a friend, admired and respected as an official, his example as a man and a statesman is one to which all Americans may turn for inspiration and emulation: Therefore be it

Resolved, **That in the death of James Wilson American agriculture has lost one of its greatest exponents and American citizenship one of its finest exemplars.**

In token of respect the flags on all department buildings will be placed at half-mast, and a copy of this resolution will be sent to the family.

RESEARCH IN AVIATION

AN addition has been made to the activities of the Imperial College at South Kensington in the establishment of a new department of aeronautics. The movement was initiated by Sir Basil Zaharoff's endowment of the University of London chair of aviation, to which Sir Richard Glazebrook was appointed, with the duty of directing the new department.

The London *Times* states that the department was established to give effect to the scheme proposed by the committee on education and research in aeronautics in their report, dated December 12, 1919, in which the opinion was expressed that the Imperial College should become the central school for advanced study in aeronautical science. The school is administered by an advisory committee of twelve members, with Sir Arthur Acland as chairman, representing the college, the air ministry, the aeronautical research committee, the University of Cambridge, the Royal Aeronautical Society, and the Society of British Aircraft Constructors.

A comprehensive scheme of instruction and training, mainly post-graduate in character, has been arranged for next session beginning in October, including special sections in aeronautical engineering, meteorology and navigation, and with the cooperation of the Air Ministry the services have been engaged of a distinguished staff of experts. Sir Napier Shaw will be professor of meteorology; Mr. Leonard Bairstow, professor of aerodynamics; Mr. A. J. Sutton Pippard will deal with the structure and strength of aircraft, and Mr. A. T. Evans with the aircraft engines. Courses of lectures

will also be given dealing respectively with airships and with navigation, while arrangements are in hand for special instruction in air-cooled engines, high-compression engines, dopes, instruments, wireless telegraphy, and similar subjects. It has also been arranged that students will carry out part of their practical training in one or other of the government establishments concerned with aeronautics.

VARIETIES OF WHEAT

The Department of Agriculture reports that the introduction of hard winter wheat from Russia into Kansas and other states of the central Great Plains area in the early seventies was an epoch-making event. The growing of these Crimean wheats, especially the Turkey and Kharkov varieties, has been the principal cause of the prosperous development of much of that section. The development and distribution of Kanred, an improved strain of hard red winter wheat, may prove equally epoch-making in the history of Kansas. Kanred is one of the most important examples of the improvement of wheat by the method of pure line selection. It is the product of a single head, selected in 1906 at the Kansas Agricultural Experiment Station. Its true value was determined only after many years of careful experiments, but as a reward not fewer than 500,000 acres were sown in the State of Kansas alone in the fall of 1919. Since 1917, Kanred has been under experiment in many states other than Kansas. Last fall many thousands of bushels were introduced into other states for commercial growing. Kanred is unusually well adapted to many of the varying conditions in the state of Kansas. Its principal advantage over Turkey and Kharkov is its resistance to some forms of both stem rust and leaf rust. It has other advantages, however, such as slightly greater winter hardiness, earlier maturity, and makes better pasture. Those factors have caused it to outyield the Turkey and Kharkov wheats in most sections of Kansas by 3 to 5 bushels per acre. The same factors may or may not be as important in other states.

To determine the varieties of Australian wheat best adapted to conditions on the Pacific coast, the United States Department of Agriculture has conducted a series of experiments which accurately ascertained the yield and quality of those varieties already of commercial importance in that region, as well as other varieties, samples of which were brought direct from Australia. In connection with the latter phase of the investigation more than 130 samples of wheat were obtained, representing 92 distinct varieties. Results from the early experiments with these wheats show that the "Federation group," consisting of three varieties, Federation, Hard Federation, and White Federation, is probably the best suited to this western region. These three varieties were compared in yield with the leading commercial wheats, including the Bluestem, Australian varieties, Pacific, White Australian, and Early Baart, and produced higher yields, according to the department's cereal specialists. Hard Federation produced the larger yields in Oregon, while White Federation did better in California. Milling experiments indicate that Hard Federation is equal or superior for milling and bread-making purposes to the leading commercial varieties now grown on the Pacific coast and also superior in this regard to Federation and White Federation.

LECTURES AT THE NEW YORK BOTANICAL GARDEN

Free public lectures are being delivered in the lecture hall of the museum building, Saturday afternoons, at four o'clock, as follows:

Sept. 4. "How to can fruits and vegetables," Professor H. D. Hemenway.

Sept. 11. "What Columbus saw in the new world," Dr. W. A. Murrill.

Sept. 18. "National losses due to plant diseases," Dr. M. T. Cook.

Sept. 25. "Dahlias and their culture," Dr. M. A. Howe.

(Exhibition of Dahlias, Sept. 25 and 26)

Oct. 2 "Nuts and other food crops from trees," Dr. W. C. Deming.

Oct. 9. "Evergreens," Mr. G. V. Nash.

Oct. 16. "Autumn colors," Dr. A. B. Stout.

Oct. 23. "Women as horticulturists," Miss E. L. Lee.

Oct. 30. "The plant life of the south," Dr. F. W. Pennell.

Free public lectures on Sunday afternoons, at four o'clock, are as follows:

Sept. 5. "Ceylon, the Pearl of the Orient," Dr. H. A. Gleason.

Sept. 12 "The vegetation of Alaska and its significance," Dr. Arthur Hollick.

Sept. 19. "Planting to attract our native birds," Dr. G. C. Fisher.

Sept. 26. "How plants get their food," Mr. Norman Taylor.

(Exhibition of Dahlias, Sept. 25 and 26)

Oct. 3. "Plant motives in Renaissance decorative art," Dr. W. A. Murrill.

Oct. 10. "Recent plant immigrants and new American plant industries," Dr. David Fairchild.

Oct. 17. "Poisonous plants in fields and woodlands," Dr. Wm. Mansfield.

Oct. 24. "House plants: their care and culture," Mr. H. Findlay.

Oct. 31. "The dehydration of foods," Dr. R. H. McKee.

Free public lectures in the central display greenhouse, Conservatory Range 2, on Saturday afternoons, at three-fifteen o'clock, will be as follows:

Nov. 6. "Palms and their products," Dr. N. L. Britton.

Nov. 13. "Tropical aquatic plants," Mr. G. V. Nash.

Nov. 20. "Tropical beverage plants," Dr. H. A. Gleason.

Nov. 27. "Bananas and their relatives," Dr. W. A. Murrill.

Dec. 4. "Tropical plants yielding starch," Dr. M. A. Howe.

Dec. 11. "Plants yielding rubber," Dr. A .B. Stout.

SCIENTIFIC NOTES AND NEWS

SIR EDWARD THORPE, emeritus professor of chemistry in the Imperial College of Science, London, has been elected president of the British Association for the Advancement of Science for the meeting to be held next year at Edinburgh. Sir Charles Parsons has been elected a trustee in the place of the late Lord Raleigh. It was found impracticable to go to Colombo in 1922, and an invitation from Hull has been accepted for that year.

DR. LEONARD G. ROWNTREE, professor of medicine in the medical school of the University of Minnesota, and Dr. Reginald Fitz, associate in medicine of the Massachusetts General Hospital, have joined the staff of the Mayo Foundation for Medical Education and Research, at Rochester, Minn. Drs. Rowntree and Fitz will be associated in the further development of research in internal medicine.

MR. W. D. COLLINS, of the Bureau of Chemistry, U. S. Department of Agriculture, has been appointed chief of the quality-of-water division of the U. S. Geological Survey.

MR. EARL P. CLARK, assistant in chemistry at the Rockefeller Institute for Medical Research, New York City, has joined the chemical staff of the Bureau of Standards.

DR. H. S. HELE-SHAW, Harrison professor of engineering in University College, Liverpool, 1886–1903, and in the university from 1903–04, has been elected emeritus professor of engineering in the University of Liverpool.

THE following degrees have been conferred by the University of Dublin: doctor of science, Sir William H. Bragg; doctor of medicine, Sir Archibald E. Garrod; doctor of law, Sir Donald Macalister.

MR. H. S. BAILEY, formerly of the Bureau of Chemistry, U. S. Department of Agriculture, resigned his position with E. I. du Pont de Nemours and Company on July 1, to take charge of research for the Southern Cotton Oil Company at Savannah, Georgia.

B. S. BUTLER has resigned from the U. S. Geological Survey, and will be associated with L. C. Graton in a study of the geological problems of the Calumet and Hecla mines.

D. H. NEWLAND has resigned as assistant state geologist of New York, and has taken a position with the Beaver Board Companies of Buffalo, New York, as field geologist and mining expert.

MAJOR LAWRENCE MARTIN, of the General Staff, and one of the map experts of the Army, who has been on duty in the Military Intelligence Division, has been ordered to report to

the secretary of state for temporary duty for the purpose of assisting in preparing a report on the proposed western boundary of Armenia. The report is intended to aid President Wilson in considering questions relating to the boundaries of Armenia. Major Martin was formerly associate professor of physiography and geography in the University of Wisconsin. He was chief of the Geographical Branch of Military Intelligence, attached to the American Commission to Negotiate Peace, from November, 1918, to December, 1919. One of his details while at the Peace Conference was that of geographer to General Harbard's Military Mission to Armenia

W. T. Thom, Jr., of the U. S. Geological Survey, who has been doing relief work in Vienna, Austria, for the past six months, has returned to Washington.

Dr. M. X. Sullivan, biochemist of the U. S. Public Health Service, gave a course of sixteen lectures on "Public Health" at the Converse College Summer School and Red Cross Institute for social workers, Spartanburg, South Carolina.

A lecture entitled "News from the stars," under the auspices of the University of California Extension Division by Dr. R. H. Aitken, astronomer at the Lick Observatory, will be delivered before the Fresno County Medical Society at the University Club, open to the public, to begin the program of lectures for the fall by the university extension division of the University of California.

Joseph Paxon Iddings, formerly geologist of the United States Geological Survey and professor of petrology in the University of Chicago, distinguished for his work on igneous rocks, died on September 8, at the age of sixty-three years.

Samuel Mills Tracy, agronomist of the United States Department of Agriculture, died at Laurel, Miss., on September 5, aged seventy-three years. Dr. Tracy was born at Hartford, Vermont, and graduated from Michigan State Agricultural College in 1868. From 1877 to 1887 he was professor of botany and agriculture at the University of Missouri, and from 1887 to 1897 was director of the Mississippi Agricultural Experiment Station. Since that time he has been attached to the United States Department of Agriculture. He was a fellow of the American Association for the Advancement of Science, in the work of which he took an active part, and a member of the New Orleans Academy of Science, and the Botanical Society of America. Among his works are the "Flora of Missouri," the "Flora of Southern United States" and numerous bulletins issued by the Mississippi Experiment Station and the United States Department of Agriculture.

Ellis L. Michael, zoologist of the Scripps Institution for Biological Research of the University of California from the foundation of the institution fifteen years ago, died at La Jolla, California on August 30. A correspondent writes: "Mr. Michael did notable work on the Chætognatha, and in the broader field of quantitative and statistical treatment of problems in marine planktology. So few are the workers in this domain of biology that his loss is specially heavy." Mr. Michael had just passed his thirty-ninth birthday.

Edward Kinch died on August 6 at the age of seventy-one years. Professor Kinch was from 1881 to 1915 professor of chemistry at the Royal Agricultural College, Cirencester.

The death is announced at Cassel, Germany, of Dr. M. Alsberg, the anthropologist, at the age of eighty years.

About forty members of the British Society of Glass Technology are visiting American glass plants. A joint meeting of the glass division of the American Ceramic Society and the British Society of Glass Technology was held in Pittsburgh on September 1, 2 and 3.

A federation of the biological clubs affiliated with the Paris Biological Society was recently formed in the laboratory of Professor Brachet in the anatomical laboratory of the University of Paris. The clubs of Brussels, Lille, Lyons, Nancy, Strasbourg, Bucharest and Copenhagen were represented. Professor Bard, of Strasbourg, was elected president and the next meeting is scheduled at Strasbourg in the autumn of 1921.

THE Air Ministry, in an official *Notice to Airmen,* according to the London *Times,* details innovations recently introduced in the dissemination of meteorological statistics and forecasts by wireless telegraphy for the use of aircraft. Reports are issued from the Croydon aerodrome on a 900-meter continuous wave each day, including Sundays, at hourly intervals between 7.35 A.M. (G.M.T.) and 4.35 P.M., the data in each consisting of observations made 35 minutes previously at the following places: Felixstowe, Croydon, Biggin Hill, Lympne, Beachy Head, Dungeness, and Botley Hill (North Downs). In addition to the usual information, the messages now include the direction and speed of the low cloud, the character of the sea-swell and the visibility towards the sea is distinguished from that over the land, the latter important feature being observed at various points along the channel coast. A statement is also added regarding the conditions prevailing on the North Downs as viewed from Biggin Hill, while at 8.25 A.M. the complete results of a pilot-balloon ascent at Croydon or Lympne are appended whenever available. Every statement is suffixed by the latest Meteorological Office estimate of the probable weather during the remaining hours of daylight. Reports of a similar character are also issued on the same wave-length from Le Bourget seven times daily, the observations transmitted in this case being derived from St. Inglevert, Abbeville, Maubeuge, Havre, and Le Bourget.

THE department of hygiene and public health at King's College, London, which offers complete courses of instruction for the various degrees and diplomas in public health, has recently been reorganized under the general supervision of Professor Simpson. Professor Sommerville, lectures on hygiene, sanitary law and administration, sanitation and vital statistics, etc., and Mr. Rhys Charles on the Food and Drugs Acts. Bacteriology and parasitology is taught by Professor Hewlett and Dr. Taylor, and the chemical laboratory work is in charge of Mr. William Partridge. The laboratories are open daily for instruction and research, and arrangements are made to suit the convenience of those engaged in practise. Weekly demonstrations on sanitary appliances and visits to places of sanitary interest are arranged. A special course on industrial hygiene is given by Dr. Legge (October to February) and courses on school hygiene are given by Dr. Malcolm (October to June).

As the part of the university extension work the Boston Teachers' School of Science will offer this fall courses in botany, geography, geology and zoology. The courses will be given by Professor W. J. V. Osterhout, of Harvard, Professor Elizabeth F. Fisher, of Wellesley; Professor George H. Barton and Professor George H. Parker, of Harvard. The school also announces its autumn course of field lessons in geology as follows: September 11, Baker Bridge; September 18, Andover; September 25, Braintree; October 2, Wayland; October 9, Orient Heights; October 16, Naugus Head; October 23, Roberts; October 30, West Quincy; November 6, Kendal Green.

UNIVERSITY AND EDUCATIONAL NEWS

THE University of Buffalo has received from O. E. Foster a gift of $400,000 for the erection of a chemistry building. It has also received anonymous gifts of $250,000 toward endowment and of a library building.

THE late Dr. J. G. Bartholomew has bequeathed to the University of Edinburgh the sum of £500, to be applied towards the foundation of a chair in geography.

RECENT appointments at Harvard University include those of Richard D. Bell, assistant professor of biological chemistry; W. T. Bovie, Ph.D., '14, assistant professor of biophysics and instructor in bacteriology; Stanley Cobb, '10, assistant professor of neuropathology; Calvin G. Page, '90, assistant professor of bacteriology; Marshal Fabyan, '00, assistant professor of comparative pathology; Joseph C. Aub, '11, assistant professor of physiology; Robert B. Osgood, '89, instructor in orthopedic surgery, and James B. Ayer, '03,

and Lesley H. Spooner, '03, instructors in neurology and bacteriology, respectively.

In the department of chemistry of the West Virginia University the following additions have been made to the teaching staff: Dr. C. A. Jacobson, professor; Dr. E. C. H. Davies, associate professor; Lily B. Sefton, assistant professor, and A. E. Owens, instructor.

New additions to the staff of the division of agricultural biochemistry at the University of Minnesota are: instructors, Arthur K. Anderson, Paul F. Sharp and G. S. Taylor; assistants, Walter F. Hoffman, Earl R. Norris, Martin W. Sandstrom, Clifton W. Ackerson and Edward F. Danielson. S. D. Wilkins, special analyst, recently resigned to enter commercial work and his position has been filled by the appointment of Mr. Arnold H. Johnson.

David F. McFarland, M.S. (Kansas), Ph.D. (Yale), formerly associate professor of industrial chemistry and metallurgy at the University of Illinois, has been appointed professor and head of the department of metallurgy in the school of mines of the Pennsylvania State College.

Dr. Joshua M. Slemons, professor of obstetrics and gynecology in Yale University School of Medicine, New Haven, has resigned and is succeeded by Dr. Arthur H. Morse.

Dr. Ardrey W. Downs, formerly assistant professor of physiology at McGill University, Montreal, has accepted the chair of physiology in the University of Alberta.

Dr. W. S. Lazarus-Barlow has been appointed to the university chair of experimental pathology at Middlesex Hospital Medical School, London.

DISCUSSION AND CORRESPONDENCE

GALILEO'S EXPERIMENT FROM THE LEANING TOWER

To the Editor of Science: Professor Cajori's article entitled "Aristotle and Galileo on Falling Bodies"[1] recalled to mind a question recently asked by a member of the department of science in this school. The question was: "Just what experiment did Galileo perform from the leaning tower of Pisa?" The writer did not know, and endeavored to find out, without success. Some notes he made may be of interest.

Poggendorf, "Geschichte der Physik," p. 224, 1879, says "Galileo dropped balls of different sizes" and gives no citation of authority.

Rosenberger, "Geschichte der Physik," 1882, Vol. I., p. 141, states that Galileo proved by experiment from the leaning tower of Pisa in 1590 that light bodies fall as fast as heavy bodies. No citation.

The same author in Vol. II., p. 16, states that Galileo let fall stones singly and tied together and they fell in the same time. Also says that Galileo dropped a 100 lb. shot and a ½ lb. shot and that they reached the ground not the width of a hand apart. No citation.

Heller, "Geschichte der Physik," Vol. I., p. 346, 1882, states that Galileo dropped from the leaning tower of Pisa, pieces of wood, lead and marble and that they fell in nearly the same time. No citation.

Cajori[2] gives a circumstantial account of the celebrated experiment and says, "One morning before the assembled university, he ascended the leaning tower, and allowed a one pound shot and a one hundred pound shot to drop together. The multitude saw the balls start together, fall together, and heard them strike the ground together." No citation.

Apparently all of the above statements have their foundation in Viviani's "Racconto Istorico di Vita di Galileo Galilei," written some time after 1654, at the request of Leopold of Tuscany. Viviani[3] states that Galileo demonstrated by repeated experiments made from the leaning tower of Pisa that bodies of different weights falling through the same medium move with equal velocity. He also states that the experiments were made in the presence of the other readers, philosophers and all the students. Viviani knew Galileo from

[1] Science, 60, 615, 1920.

[2] "History of Physics," p. 32, 1899.

[3] "Opere di Gal.," Edizione Nazionale, XIX., p. 606.

1639 to Galileo's death in 1642. In 1639 Viviani was seventeen years old.

No account by Galileo himself is to be found in the Edizione Nazionale of his works, if the elaborate index is to be trusted. Further as Gerland, "Geschichte der Physik," p. 316, 1913, points out, Galileo in his treatise "De Motu," which dates from the time of his readership in the University of Pisa, cites experiment from a high tower as proving that wood at the beginning of its fall moves more rapidly than lead, but that a little later the lead will pass the wood and will precede the wood by a great space. Galileo further states, "and on this I have made experiment frequently."[4]

Renieri (born in 1606, knew Galileo from 1633 to 1642 and during that time wrote Galileo at least sixty letters) wrote a letter to Galileo dated March 13, 1641,[5] in which he gives an account of some experiments performed by Renieri from the leaning tower of Pisa. Renieri dropped a sphere of wood and one of lead the same size; on reaching the ground the lead was three "braccia" ahead of the wood. He also dropped a cannon ball and a musket ball and on reaching the ground the cannon ball was a palm ahead. Renieri makes no reference to Galileo's experiments, which is difficult to explain except on the ground that he had never heard of them.

Realizing the slippery nature of historical deduction, I draw no conclusion except this, that we do not know exactly what experiment Galileo performed from the leaning tower.

EDW. A. PARTRIDGE

WEST PHILADELPHIA HIGH SCHOOL

THE BOOMING LIZARD OF AUSTRALIA

IN July, 1913, I was hunting in the Cove's River Ranges with two companions—Andrew and John Duncan, of Megalong. We found a black-and-yellow banded monitor lizard about five feet long concealed in a crevice on the face of a wall. We had no intention of injuring him, but out of mischief Andrew Duncan suggested that we should make him yell. I was sceptical, but both men assured me that the lizard could, and would yell under persuasion. On condition that there should be no cruelty, I consented to a demonstration. They laughed at the idea of cruelty. Andrew picked up a stick and began poking the reptile in the ribs and tickling him under the arm. It stood it for a while, merely squirming closer down in the crevice, then, having had enough of it, blew himself out and emitted a most comically doleful bellow that could be heard several hundred yards away. This he did repeatedly until we had laughed ourselves tired. It was funny, on looking back after we had gone fifty yards, to see the lizard stick his head around the corner to make sure that we had really departed.

I have had for the last four years at my hunting-box on the Naltai River, a tame monitor whom I have called "Joseph" on account of his coat of many colors, and who is the interesting companion of my solitude—and incidentally keeps the snakes away. With the above in my mind, I experimented on him. I found him camped under the bench beneath the window, and irritated him with the end of a stick. He did as the other had done—filled himself with wind and then emitted it in a prolonged bellowing groan. By the way, our "bookbook owl" does much the same thing. He fills himself nearly to bursting in a succession of gasps, and then says "Hoo-hoo hoo" till he has no more breath, then fills up again.

WALTER H. BONE

LINNÆAN SOCIETY OF NEW SOUTH WALES, SYDNEY

[4] Op. Ed. Naz. I., 334.

[5] Op. di. Gal. Ed. Naz., 18, p. 305.

QUOTATIONS

THE BRITISH ASSOCIATION

THE British Association begins its annual meeting at Cardiff to-day. Our correspondents report that the increase in railway fares has not reduced the attendance below that of a fair average, and that the arrangements made for the housing of the visitors and the accommodation of the sectional meetings are excellent. By a useful innovation the daily

Journal has been replaced by a single issue covering the whole meeting. Clearly it will be a strenuous time. To-day eleven of the twelve sectional presidents deliver their addresses, on subjects ranging from the constitution of the stars to the intensive cultivation of gooseberry bushes. The detailed proceedings of the sections in the morning and the afternoon should be full of interest. An announcement is to be made as to the third of the three practical tests proposed by Einstein for his new theory of relativity, two having been already successfully passed. The chemists are to consider the production of alcohol for industrial purposes. The geology of the district and the possible effect of the narrow valleys in provoking feelings of imprisonment and isolation on their thronged population are to engage the attention of the geologists and geographers—unfortunately, in two separate sections. The economists are to discuss decimal coinage, the Danish credit system, and the business side of agriculture, while the agriculturists are busy over the growing of potatoes. The importance of psychology, the mental effects of alcohol, and training in citizenship are all on the day's program. In the evening, Professor Herdman, president-elect, will be inducted into the chair, and will discourse on Oceanography and Fisheries. The following days, if not quite so arduous, are at least to be well filled. Since the Association held its first meeting, at York in 1831, there has been a great advance in science, and an increasing specialization of its branches. At first there were only six sections, and the next year, at the Oxford meeting, these were reduced to four, dealing respectively with mathematics and physics, chemistry, mineralogy and electricity, geology and geography, biology. By 1855 they had grown to seven; they are now twelve, and a proposal for still further sub-division is to be discussed. We wonder if it is all gain. The reverse tendency is also at work, and several sections are to combine for occasional joint discussions. There is much to be said in favor of a concentration at the annual meetings on subjects whose problems concern many different branches of science and require illumination from many points of view.—The London *Times*.

SPECIAL ARTICLES

EXPERIMENTS IN THE TRANSPLANTATION OF THE HYPOPHYSIS OF ADULT RANA PIPIENS TO TADPOLES

The writer has for some time past been engaged in experiments upon the extirpation of the hypophysis and the thyroid glands of tadpoles. These experiments have yielded interesting results. Absence of the thyroid gland wholly prevents metamorphosis while the removal of the pars buccalis of the hypophysis, *i. e.*, all but the posterior lobe brings about the following results:

1. Failure to metamorphose.
2. Retardation of growth in size.
3. A striking change in color from black to a silver white.
4. Lowered resistance to unfavorable conditions.

Experiments in transplanting of the hypophysis were undertaken by the writer two years ago, but failed because of faulty technique, and were for the time being abandoned owing to press of other work. This year the experiments were carried through with surprisingly little difficulty and have given such striking results that it seems well worth while to offer a brief and necessarily rather superficial account of them at this time. These experiments are based on 384 operations upon tadpoles.

It is well known that the hypophysis is composed of four elements: the anterior lobe, intermediate lobe, pars tuberalis, and posterior lobe. All but the last named come from the same embryonic anlage—the portion that has been removed in the extirpation experiments mentioned. For the sake of brevity we shall speak of these as "pituitaryless" tadpoles. For the present work three out of the four lobes—all except the pars tuberalis—were employed. In each case a fair amount of care is exercised to prevent infection but these precautions are in no case perfect. Greatest reliance is placed upon the remarkable resist-

ance of the tadpoles to infection. In each case the portion transplanted is placed in a pocket under the skin above the right eye. A delicate knife made from a needle ground to a cutting edge is used in making the pocket into which the piece of material is thrust.

The chief aim of these experiments is to study the several functions of the different portions of the hypophysis. It is quite easy to separate the anterior lobe from the other portions but the intermediate and posterior lobes are tightly applied together although the difference in texture makes it quite easy to distinguish them. In a large portion of the experiments these lobes were both transplanted together. In other series they were laboriously dissected apart and separately transplanted.

The experiments show in clearest fashion that each part functions quite differently from the others.

1. The anterior lobe transplanted to normal, to pituitaryless, and to thyroidless tadpoles in each case produces a marked acceleration of growth so that the tadpoles thus treated are conspicuously larger than those into which the other parts of the hypophysis have been transplanted. They are larger than normal controls and larger than controls into which muscle tissue has been transplanted in the same way and at the same time as the above operations.

The anterior lobe also produces a marked acceleration in the development of the hind legs. This happens to be most conspicious in the pituitaryless specimens probably because they were the first operated; but at the date of writing—June 14th—the same appears to be true of the normal and the thyroidless specimens into which this lobe has been transplanted. The white pituitaryless tadpoles into which this lobe has been transplanted show not the slightest tendency to return to the original black color except for a slight temporary tendency at the beginning. This may be due to the adhesion of particles of the intermediate lobe or to a certain amount of secretion that had diffused from the latter. It soon clears up however.

2. Normal tadpoles into which the intermediate lobe is engrafted become much more darkly colored than the controls, while those which have been made to turn white as a result of removal of the anlage of the hypophysis exclusive of the posterior lobe are made to change back from white to black when the intermediate lobe is engrafted into them. There is not the slightest doubt that this lobe is responsible for the conspicious color changes controlled by the hypophysis. This return to the black color takes place slowly, being scarcely completed inside of ten days. Specimens into which the intermediate and posterior lobes together have been transplanted show this phenomenon of deepening of the black pigmentation as well as those into which the intermediate lobe alone has been engrafted.

3. Tadpoles into which the combined intermediate and posterior lobes are transplanted show not only the color change mentioned above but they also suffer a marked contraction of the body walls. Within twelve hours they appear very emaciated. This characteristic gradually disappears, in the course of ten days. These tadpoles show apparent retardation of growth. When the intermediate and posterior lobes are dissected apart and transplanted separately it is seen that this phenomenon is wholly due to the posterior lobe. It is probably caused by the well-known property that this portion of the hypophysis possesses for bringing about muscular contraction. The details of this will need further study. The posterior lobe does not produce a restoration of the black color to pituitaryless tadpoles.

In summing up it may be said that although we have not taken the pars tuberalis into account and can not make a complete analysis of the functions of the different parts of the hypophysis until we do, the following conclusions are justified.

1. The anterior lobe of the hypophysis stimulates growth and metamorphosis.

2. The intermediate lobe is very largely if not wholly concerned in regulating such color changes as are controlled by the hypophysis.

3. The posterior lobe causes marked contraction of the body walls and at least apparent retardation in growth.

Bennet M. Allen
Department of Zoology,
University of Kansas,
June 14, 1920

THE AMERICAN CHEMICAL SOCIETY. VII

The determination of the specific heat of heavy mineral oils: Herbert Bailey and C. B. Edwards. There is apparently very little information available concerning the specific heats of heavy mineral oils at temperatures of 150°–250° C. Such information is often needed in connection with the proper design of plants in which such oils are used as the heat transfer medium. In order to ascertain the specific heat of a particular oil over short temperature intervals between 120°–235° C. a simple apparatus was devised, consisting of a quart vacuum jacketed fruit jar, a small electrically heated resistance coil, and a motor driven glass stirrer. After obtaining, experimentally, the water equivalent of this calorimeter, and its cooling curve, measurements were made of the rate of rise in temperature of a definite amount of oil in the apparatus when heated with a nearly uniform input of electrical energy. For the particular oil varied from 0.543 for the range 120°–140° C. to investigated, it was found that the specific heat 0.630 between 215°–235° C.

The freezing points of mixtures of sulphuric and nitric acids: W. C. Holmes. Freezing points were determined on three sets of mixtures of sulphuric and nitric acids, having total acidities of 100 per cent., 95 per cent. and 103 per cent. respectively, in which the content of nitric acid was varied from 0 to 50 per cent. In the case of the 100 per cent. acidity mixtures, the freezing point of the sulphuric acid was depressed by the addition of nitric acid until a minimum was reached, when 5.5 per cent. HNO_3 was present. On further increasing the nitric content, a sharp rise in the freezing point to 2.3° took place, with a nitric content of approximately 11 per cent. It seemed probable that the composition at this maximum represented a definite chemical compound between sulphuric and nitric acids, the acids being present at that point in the proportion $5H_2SO_4$–HNO_3. The freezing point curve showed a similarity to that of sulphuric acid and water. The freezing point curve for the 95 per cent. and 103 per cent. mixtures showed a general similarity to that of the 100 per cent. acids, and possessed similar maxima and minima. They were complicated by the presence of a third constituent, water and free sulphur trioxide, respectively.

Strength and velocity of detonation of various military explosives: W. C. Cope. The strength of various military explosives that were used or proposed for use during the European War has been determined by the ballistic mortar in terms of TNT which is taken as a standard. Several explosives not used for purely military purposes are also given for comparison to aid in fixing the comparison in the mind. The velocity of detonation of several explosives confined in iron pipes $1\frac{3}{16}$" inside diameter was determined by the Dautriche method and compared with TNT. The velocities ranged from 5,870 to 7,600 m./sec. Several explosives contained in three inch base detonating shells were detonated and the resulting fragments compared using TNT as a standard.

Potash and reconstruction: J. W. Turrentine. Since the signing of the armistice, when most of the American potash-producing plants suspended operations, imports of foreign potash have not been great enough to supply America's requirements even at the lower prices prevailing. The situation is critical, for the American industry has been demoralized and the foreign industry has not been able to function. This is bad enough, but there is the additional fact which makes matters many times worse; namely, Germany is quoting to the German farmer potash at a price of $2.70 to $3.00 per unit which was formerly sold to the American farmer at 60 cents wholesale, delivered free. On this basis the American farmer will have to pay Germany seventy-five millions per annum for even that quantity of potash used ten years ago and for which he paid an annual bill of fifteen millions. Can American agriculture stand this price for an essential fertilizer ingredient and can it afford to do without?

The experimental kelp-potash plant of the U. S. Department of Agriculture. Second report: J. W. Turrentine. The processes under development have been brought to the point where the only potash produced now is high-grade muriate (85 per cent. KCl). This is being yielded and marketed in such quantity as to pay a very substantial part of the entire expenses, both operating and experimental, of the enterprise. By-products, bleaching carbon and iodine, are being commercial-

ized, and in the immediate future will be turned out in such quantity as to increase proceeds from sale of products to a point where all expenses will be equaled and a profit established. In the meantime other by-products of proven value, such as ammonia, are being developed and soon will be put on a commercial basis.

The reactions of coal sulfur in the coking process:[1] ALFRED R. POWELL. Previous work has indicated that sulfur exists in coal in three typical forms—pyrite or marcasite, sulfates and organic sulfur. A study of the changes which these forms undergo during coking has been made on a variety of coals, and the following five classes of reactions established. (1) Complete decomposition of the pyrite and marcasite to ferrous sulfide, and hydrogen sulfide. (2) Reduction of sulfates to sulfides. (3) Decomposition of the organic sulfur to form hydrogen sulfide. (4) Decomposition of a small part of the organic sulfur to form volatile organic sulfur compounds. (5) Disappearance of a portion of the ferrous sulfide and pyrrhotite, the sulfur apparently entering into combination with the carbon.

The desulfurizing action of hydrogen on coke:[2] ALFRED R. POWELL. A complete study has been made of the efficiency of hydrogen and gases containing hydrogen as desulfurizing agents when passed through coal in the process of coking. The effect of the hydrogen on the removal of sulfur from coke was very noticeable, in some cases nearly all of the sulfur being removed as hydrogen sulfide during a period of three hours. The ordinary sulfur coking reactions were affected in two ways by the passage of hydrogen through the coking mass: (1) the pyrite was caused to decompose at a much lower temperature, (2) the coke sulfur, which is presumably in combination with carbon, was elimination at an almost constant rate at the higher temperatures of the coking process. The size of the particles of coal did not seem to affect the rate of evolution of hydrogen sulfide. For gases containing hydrogen, the desulfurizing efficiency seemed to be proportional to the partial pressure of the hydrogen.

The analysis of sulfur forms in coal:[3] ALFRED R. POWELL. An investigation of the applicability to a variety of coals other than those from Illinois of the Powell and Parr method of analysis for the forms of sulfur in coal (Bul. 111, University of Illinois Engineering Experiment Station, Urbana, Ill., 1919). A complete study was made of the sulfur as it existed in the following coals—Upper Freeport, Pa., Pittsburgh seam, Pa., Pocahontas No. 3, W. Va., Letcher Co., Ky., Morgan Co., Tenn., and Cherokee Co., Kan. The method of analysis gave excellent results when applied to these coals. In the determination of pyrite, the iron-sulfur ratio checked in every case with the theoretical. After the extraction of the pyrite and sulfates, the remaining sulfur was proved to be organic in nature.

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[2] Published by permission of the director of the Bureau of Mines.

[3] Published by permission of the director of the Bureau of Mines.

DIVISION OF WATER, SEWERAGE AND SANITATION

J. W. Ellms, *chairman*

W. W. Skinner, *secretary*

Sewage disposal committee of the National Research Council: EDWARD BARTOW. This committee has been appointed to consider the many problems connected with sewage disposal and the recovery of valuable ingredients. The problems to be considered include:

1. Colloids and their effect on sewage disposal.
2. Fertilizer value of sewage sludges.
3. Content and value of grease.
4. Special processes of sewage disposal.
5. Bacteriological problems.
6. Pacific Coast problems.
7. Sewage experiment stations.
8. Primary sewage treatment including use of screens and tanks.

The committee asks the cooperation of officials, engineers, chemists, bacteriologists and others who are in charge of or connected with treatment plants or experiment stations, or who may be consultants on the design, construction or operation of such plants. Those willing to assist in the work may notify the chairman, E. Bartow, Urbana, Illinois.

Relationship of H-ion concentration of natural waters to carbon dioxide content: R. E. GREENFIELD and G. C. BAKER. The H-ion concentration of most natural waters may be fairly accurately calculated by use of the simple mass law equation of the primary ionization of carbonic acid. Bicarbonates and free carbonic acid are determined as directed in Standard Methods of Water Analysis A.P.H.A. Bicarbonates are considered 85 per cent. ionized and the carbonic acid determination is corrected for the free carbonic acid still left in the solution at the endpoint of the titration. When the free carbonic acid is expressed in parts

per million of CO_2 and the bicarbonates in parts per million of $CaCO_3$ this equation becomes:

$$\text{H-ion Concentration} = \frac{4.0 \times 10^{-7}}{HCO_3} CO_2 + 1 \times 10^{-8}.$$

When both free carbonic acid and bicarbonates are expressed in either parts per million CO_2 or cc.CO_2 the equation becomes:

$$\text{H-ion Concentration} = \frac{3.5 \times 10^{-7}}{HCO_3} CO_2 + 1 \times 10^{-8}.$$

Preparation of ammonia-free water: G. C. Baker. Ammonia free water may be prepared by passing distilled water through permutit. This method has advantages over other methods in (*a*) ease of operation and (*b*) production of large quantities at minimum expense. Its disadvantages are (*a*) gives a water of higher mineral content and (*b*) does not remove nitrate, nitrate or albuminoid nitrogen. Indications are that American Permutits, except the especially prepared Folin Permutit will not quantitatively remove ammonia, but the English and German Permutits seem satisfactory.

Sewage treatment at Fort Myer, Virginia: J. W. Sale and W. W. Skinner. Sewage at Fort Myer is treated by settling, septicization, and aeration. The system cost about $8,000, was designed for a population of 2,000 and was used a model for plants at other cantonments. A chemical and bacteriological investigation extending over a period of six days showed that the effluent was stable and sludge inoffensive. It is believed that this type of plant warrants the consideration of small towns which contemplate installing sewage disposal systems.

The nitrate content of five hundred waters which were considered safe from a bacteriological standpoint: M. Starr Nichols. Nichols reports the nitrate nitrogen findings of 767 ground waters which were found to be safe bacteriologically. 81.7 per cent. of the waters examined had nitrate nitrogen values of between 0 and 5 parts per million, and 56.8 per cent. gave nitrate nitrogen values of between 0 and 1.0 part per million. He cites extracts and data of other investigators and points out that his work as well as do that of other workers, show that high nitrates are not a component of normal safe waters. He cites instances which show that a well may be subject to pollution and yet not be detected by bacteriological methods. The evidence indicates, so the author believes, that the nitrate determination should be made in addition to the bacteriological examination of every ground water and if found in greater quantities than 5 parts per million the source should be considered unsafe until a competent sanitary survey shows no possible source of pollution.

Seasonal variations of bacterial flora during filtration process: Harry E. Jordan. Following the operation of a water purification plant in the central states over a period of sixteen years—a series of some 50,000 examinations in a 5-year period is summarized with relation to seasonal ratios and variations by various types of organisms present. This data shows: (1) Bacterial concentration of all types studied, and the proportion of all types which are of the Coli group, is inversely proportional to the temperature. (2) Both sedimentation and filtration exercise a selective action against organisms of the Colon group and sterilization with chlorine products exercises a remarkably increased selective action against these organisms. (3) Of the total number of Coli type organisms present the fecal subtype survives purification processes—step by step—in increasingly less proportion as the temperature rises.

A study of sewage and trade wastes at Bridgeport, Conn.: W. W. Skinner and J. W. Sale. The investigation covered a period of one year and was made in cooperation with the Bureau of Fisheries in the interests of fish and shell-fish life. Dissolved oxygen data were obtained and the composition of about twenty effluents determined. Metals and acid from copper mills and waste dyes from textile mills were the chief problems given consideration. The water in the harbor is toxic to oyster larvæ. Remedial measures are contemplated.

Charles L. Parsons,
Secretary

(*To be continued*)

SCIENCE

A Weekly Journal devoted to the Advancement of Science, publishing the official notices and proceedings of the American Association for the Advancement of Science

Published every Friday by

THE SCIENCE PRESS

LANCASTER, PA. GARRISON, N. Y.
NEW YORK, N. Y.

Entered in the post-office at Lancaster, Pa., as second class matter